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The Effects of School District Consolidation on Educational Cost and Quality

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We examine the effects of both whole-grade sharing and administrative consolidations of local school districts in Iowa in the 1990s, the majority of which were induced by state fiscal incentives. We find no effects of either sharing or consolidation on the pupil-teacher ratio, enrollments, or dropout rates. In terms of revenues, we find evidence of temporary increases in state aid, as predicted by the state incentives. This increased state aid, however, is not offset by changes in local revenue and thus total revenue increases. We find a corresponding increase in local expenditures, although this increase was smaller than the increase in revenues, resulting in an increased surplus. Although we lack detailed quality data on student outcomes, these findings suggest an absence of efficiency gains from either whole-grade sharing or consolidation.

Keywords: *school district consolidation; school district reorganization; whole-grade sharing*

1. Introduction

Whether small school districts should consolidate into larger ones has long been a contentious issue in the United States, where the number of school districts plummeted from around 130,000 in the early 1920s to just below 15,000 today. Proponents of consolidation (typically bureaucrats, professional educators, and some elected officials) argued that by consolidating, districts would gain from economies of scale: high schools could

offer more subjects, elementary schools could separate classes by grade level, and the quality of education could generally be improved at lower costs in larger consolidated schools and districts than in smaller ones. Local residents, on the other hand, revealed through their frequent votes against consolidation that they preferred local control over the types of schools their children attended, who their children's classmates would be, and the determination of local tax rates to their own estimation of the potential efficiency gains so touted by consolidation's proponents. Before any given consolidation, neither local voters nor state policymakers know with certainty how large gains from scale will be; furthermore, the two groups may have diverging perceptions of their magnitude.

In this article, we examine a recent set of school district consolidations in Iowa, in which the number of districts statewide fell from 436 in 1985 to 371 in 2001, to examine how changes in district scale relate to student outcomes and school finances. Our unit of observation is the post-consolidation school district, with variables aggregated up to this unit before consolidation occurs. This allows us to see how outcomes differ over time by whether a set area is comprised of two districts (pre-consolidation) or one (post). State fiscal incentives for reorganization prompted many of these consolidations. There were, however, many districts that chose not to consolidate despite the presence of these incentives; we therefore emphasize that our results identify the effects of consolidation for districts that voluntarily chose to consolidate rather than any uniformly causal effect of consolidation. Identifying the magnitude of any such gains from scale is useful for several reasons. First, it is necessary in evaluating whether the magnitude of incentives paid out to districts by the state of Iowa was justified by the efficiency gains of the resulting consolidations. It also can be interpreted as a lower bound of how local voters value their reduced autonomy after consolidation. Finally, it informs ongoing policy discussion about optimal district size.

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The consolidation of school districts in the United States has potentially important implications for efficiency and equity in public education. If schools and/or districts can benefit from economies of scale, consolidation will increase efficiency in the education production function. Conversely, some have argued that large school size likely creates inefficiencies because teachers will not know individual students as well, and students then will be less motivated to perform. Barker and Gump (1964) are near the beginning of an extensive literature making this argument. Berry and West (2005) find that workers born in states with smaller schools earn higher wages than those in states with larger schools: while they found larger districts were correlated with higher wages, the concurrent influence of larger schools outweighed those benefits. Recent reform efforts by the Bill and Melinda Gates and Annenberg Foundations have devoted considerable resources to making large schools into multiple smaller "schools-within-a-school." Finally, with the bulk of education funding historically raised at the local level, if these tax bases become more economically heterogeneous, there will be more redistribution in the system of education finance.[1]

Despite the magnitude of this shift in school and district organization, little is known about its impact on educational outcomes. In a review of the literature on school district economies of scale, Andrews, Duncombe, and Yinger (2002) conclude that "despite massive consolidations of school districts in the United States, there is little convincing evidence on how consolidation actually affects school districts in the long-run" (p. 256). This dearth of evidence is due in part to the historical nature of the phenomenon and the lack of widely available student outcome data at the district level until recent years.

Consolidation remains a salient policy issue in rural areas today. In March 2004, the Arkansas legislature required all districts with fewer than 350 students to merge. Governor Mike Huckabee, who initially had pushed for requiring more high school closures than the final legislation required, "called [it] the most 'hellish' experience of his political career" (Richard 2004, 34). Governor Joe Manchin III of West Virginia, elected in 2004, took the other side of the argument, fighting for laws to cap the length of school bus rides and "preserving . . . rural schools wherever and whenever possible" (Richard 2005, 34). Knowing how large gains from scale actually are can inform such policy, as well as how jurisdictions are created in areas of new development.

The remainder of the article is structured as follows. In section 2, we provide background information on the consolidation and school finance

legislative environment in Iowa. Section 3 describes the empirical strategy. Section 4 presents the results, and section 5 concludes.

2. Background on Consolidation in Iowa

Iowa experienced an intensive spate of consolidation in the 1990s. The bulk of this consolidation activity, in the early 1990s, was the direct result of financial incentives from the state.[2] Some districts chose to consolidate before or after the incentives were offered, and not all eligible districts chose to take up the incentives. All districts that consolidated, with or without the state incentives, chose their own consolidation partners. Guy Ghan, a former reorganization consultant to the Iowa Department of Education, characterizes the period of consolidation in the 1990s as a "natural" movement in a series of state reports (Ghan 2005).

A brief background on the school finance rules in Iowa at that time is necessary for understanding the consolidation incentives provided by the state. Iowa's school finance system is based on foundation grants. Local revenue to supplement these grants is subject to per-pupil expenditure caps based on the district's historical spending levels and a state-determined rate of allowable cost growth. This system of school finance limits the fiscal capacity of districts to respond to consolidation, other than through "enrichment levies." These levies are capped at a set percentage (changing over time) of the statewide designated cost per pupil.

2.1 Whole-Grade Sharing

Whole-grade sharing was an important precursor to the mergers we are investigating. Under whole-grade sharing (WGS), which Ghan (2005) characterizes as beginning "spontaneously" and "abruptly" in 1985, two districts agreed to share one high school. In some cases, the district retaining its high school sent none of its own resident students out of the district in exchange (one-way sharing), but more commonly, the district losing its high school received junior high or middle school students from the high school district in exchange (two-way sharing). The number of districts engaged in WGS grew from two in the fall of 1984 to 111 in the fall of 1991.

In the late 1980s, the state began offering fiscal incentives for districts to engage in WGS. These incentives gave additional weight to students in shared grades (similar in process to giving disabled students additional weight) in determining the enrollment count for state funding to the district. Specifically, students in WGS arrangements counted as 1.1 "regular"

students. It is important to note that these state incentives were offered after a large number of districts already had entered into WGS arrangements. The state's position was that these incentives were there to encourage small districts to help themselves by sharing grades, and not to promote WGS as a first step on the road to consolidation (Ghan 2005).

Several historical factors also contributed to the timing of the emergence and popularity of WGS. The farm crisis in Iowa in the 1980s lowered tax bases and enrollments, making it difficult for already small school districts to cover their fixed costs. WGS, unlike consolidation, can be implemented by elected school boards in Iowa without direct voter approval. It is also reversible by a single district, while reversal of consolidation would require approval from both districts affected. WGS was therefore easier, both logistically and politically, to implement quickly in the face of budget crises than consolidation would have been. The cost of WGS varied by the specific terms negotiated in each case. The state allowed the sending district to retain up to 50 percent of the allocated amount per pupil sent to a wholly shared grade in another district. Some districts would accept students through WGS with less than 100 percent of their funding; part of their motivation was defensive, in wanting to secure WGS arrangements with the sending district before another district could do so. Falling enrollments meant that even the larger district in each sharing pair, which typically retained the high school under WGS, faced growing pressure to increase scale and lower fixed costs per pupil.

2.2 Full Consolidation during the 1990s

While the state government preferred WGS to lone small districts, it passed school finance legislation effective in 1991, which favored full consolidation of districts over WGS. These incentives applied to school districts voting by November 30, 1990 to make their consolidations effective between July 1, 1991 and July 1, 1993. The largest legislative incentive was a five-year reduction in the foundation tax rate.[4] This reduction in the foundation tax rate translated into about $5,100 per pupil over a five-year period for the average district (using the inflation rate as the discount rate in the calculation).

The second major incentive eliminated additional weights for students in WGS arrangements, but allowed school districts consolidating effective 1991-1993 to continue to weight their enrollments according to the proportion of students previously in WGS for five years after merging. This would yield a gain of about $200 per pupil for districts consolidating.

Nearly all districts consolidating in Iowa in the 1990s had been engaged in WGS arrangements prior to merging. Both the foundation tax rate reduction and continued use of supplemental WGS weights gave districts an incentive to consolidate effective 1991–1993.

Districts responded strongly to these time-specific incentives. From 1966, when our administrative data reporting consolidations by year begin, through 1990, there were zero to three consolidations per year (with 1966 the only year with more than two). In 1991, the first year for which districts received financial bonuses for consolidating, there were four consolidations. This rose to seven consolidations effective in 1992, and twenty consolidations effective in 1993. This was followed by three more years of higher than average activity in 1994 through 1996, though districts whose consolidations first took effect in these years were not eligible for the incentives.[5]

2.3 Why Consolidation?

Why did the state offer expensive incentives to move districts from WGS, where any school-level economies of scale would already be in effect, to consolidation? Did state policymakers expect that district-level administrative economies of scale were large enough to warrant the cost to the state? Ghan (2005), who conducted reorganization studies for the state, describes Iowa as a "neat, organized kind of state" that "wanted to wrap it up" and maintain a "direct chain of command" rather than the messier organizational structure of WGS. All state publications that we reviewed comment only on the benefits of consolidation as opposed to operating two distinct districts rather than specifically outlining perceived benefits of consolidation relative to WGS.

Numerous state publications and media coverage suggest that the state legislature promoted consolidation because they believed it would improve school quality, and do not reflect any belief that it would significantly reduce per-pupil costs. The state department of education publishes an *Annual Condition of Education Report,* which reports the distribution of curricular programs offered, achievement levels, and fiscal status by district size. This report presents descriptive tables and figures, without testing for statistical differences across enrollment categories. Smaller districts are shown to have more limited high school subject offerings, lower ACT scores, and lower preschool enrollment rates (Iowa DOE 2005). These types of statistics are presented as arguments for eliminating small school districts; for example, the *Des Moines Register* published an editorial in 2005 titled "365 School Districts Are Picture of Inefficiency" in which it

described the findings of the most recent annual report. Such arguments tend not to highlight other findings, such as the fact that small districts report lower high school dropout rates and higher compliance with No Child Left Behind than larger districts.

Nearly all consolidations in our sample were preceded by whole grade sharing (WGS) arrangements for the relevant districts. This means that it would be possible for consolidation of districts to result in no changes in enrollment levels by school, although it could still yield changes in administrative scale. The two-way sharing districts were already sharing superintendents as well as schools, removing major potential political opponents to consolidation, and therefore could be expected to realize little cost savings in the move from WGS to consolidation. Two-way sharing districts each maintained their own (unpaid) school boards and paid board secretaries. The relatively inexpensive position of board secretary is one easily identifiable cost that could be eliminated switching from WGS to consolidation.

3. Empirical Strategy

We first consider net effects of consolidation on fiscal and educational outcomes (implicitly comparing consolidation to WGS, its usual predecessor), then add a control variable for WGS to explicitly compare effects of full administrative consolidation to effects of WGS, then finally distinguish between effects of consolidation *per se* and consolidation eligible for the state incentives (defined by the timing of the consolidation).

Our question is essentially whether it matters if two adjacent districts operate as one or two administrative entities (keeping in mind that two administrative entities may still realize some instructional economies of scale under WGS). To answer this, we aggregate district-year level observations so that the geographic boundary of each observation *in every year* corresponds to the boundary of an Iowa school district in 2001, the final year included in our data.[6] For example, Colo and Nesco merged in 1991 to form Colo-Nesco. We compare variables like local revenue per pupil in Colo-Nesco post-consolidation (from 1991 on) to the enrollment-weighted average of local revenue per pupil in Colo and Nesco before 1991.[7] There is one combined pre-consolidation Colo-Nesco district observation in 1990, with values we sum up from the component Colo and Nesco districts; in 1991, after consolidation, there is one merged Colo-Nesco observation. We do not need to manipulate the post-consolidation data, as they are reported at the current district level in each year.[8] For each "final" district such as Colo-Nesco, we know whether the composition changed

over the 1986-2001 time period, and if so, in which year.[9] We can then construct variables for each district-year level observation indicating how long ago the consolidation took place and whether the consolidation was eligible for state incentives, which will be our main independent variables.

3.1 Baseline Specification

Our estimation strategy is straightforward: we test for the impact of consolidation on a variety of dependent variables, encompassing district finances, school inputs, and student outcomes. Because we have structured our unit of observation to be a district as it exists at the end of the time period and therefore is constant over time, we are able to include fixed effects for these "ultimate" districts. This strategy allows us to ask whether it matters if a set geographic area encompasses one or two jurisdictions. We include year fixed effects and district-specific time trends in all specifications as well. We apply this specification to a number of dependent variables. The estimating equation (1) below represents our ideal estimation, in which the effect of the merger on some type of student output is identified, given district spending:

$$\frac{output_{d,t}}{spending_{d,t}} = \alpha + \beta^{*} MERGED_{d,t} + \delta_d + \phi_t + \gamma_d t + \varepsilon_{d,t} \quad (1)$$

We estimate robust standard errors, clustered at the district level.

3.2 Consolidation versus Whole-Grade Sharing

Because we include district fixed effects and district-specific time trends in equation (1) and because most districts implemented WGS prior to consolidating, the estimated effect of consolidating generally represents the impact of consolidation when introduced into a system of WGS, rather than the impact of consolidation introduced into a system with no instructional sharing. We therefore next estimate versions of equation (2) below, in which we add a dummy variable for WGS to equation (1):

$$\frac{output_{d,t}}{spending_{d,t}} = \alpha + \beta^{*} MERGED_{d,t} + \eta^{*} WGS_{d,t} + \delta_d + \phi_t + \gamma_d t + \varepsilon_{d,t}. \quad (2)$$

3.3 How State Fiscal Incentives Affect the Impact of Consolidation

To partially distinguish the mechanical effects of the mergers via the state incentives from more fundamental effects, we next allow the effect

of consolidation to vary by whether the district-year observation is of a consolidation that may have been induced by the state fiscal incentives—that is, whether the consolidation occurred between 1991 and 1993. We further classify consolidations by how recent they are: district-year observations within five years of their consolidation date and still eligible to collect financial incentives are categorized as "new" mergers while those observations merging at least six years ago are "old." Equation (3) below allows these four types of consolidations to have heterogeneous effects on our set of district outcome measures:

$$\frac{output_{d,t}}{spending_{d,t}} = \alpha + \lambda_1 {}^{*} Merged\ Ever + \lambda_2 {}^{*} Merged\ 1\ to\ 5\ Years\ Ago_{d,t} + \lambda_3 {}^{*} Merged\ 1991\ to\ 1993_{d,t} + \lambda_4 {}^{*} Merged\ 1\ to\ 5\ Years\ Ago_{d,t} {}^{*} Merged\ 1991\ to\ 1993_{d,t} + \delta_d + \phi_t + \gamma_d t + \varepsilon_{d,t} \quad (3)$$

Again, because more districts were eligible for the consolidation incentives than merged, we caution that these specifications can estimate the impact of consolidations *for the set of districts we observe consolidating.* In related work (Gordon and Knight 2006) we estimate that eligibility for state incentives is an important determinant of consolidation activity, but also show that a number of eligible mergers do not take place.

3.4 Data Sources

Administrative data on school district consolidations are from the Iowa Department of Education. These data list the date on which each consolidation goes into effect, the names and Iowa state identification numbers of the districts merging, and the name and Iowa state identification number of the new district formed. We generate a dummy variable for WGS that represents our best estimate of whether the district was engaging in any WGS behavior in that year, based on discrepancies between grade-level enrollment at the school level (which represents all students residing in any geographic district attending that school) and at the district level (which represents only students residing in that district).

Fiscal data on districts are from the F-33 School District Financial Data files. All districts in Iowa reported major fiscal variables consistently from 1986 to the present. We also use several current spending variables that are reported in Iowa beginning in 1991: physical plant maintenance, services, and administration. We construct a surplus variable equal to total revenues net of revenues from other school systems, minus total current spending; we

exclude revenues from other school systems from revenue because payments to other school systems are not part of current spending. We use as many years of data as we have for each variable, so variables available since 1986 have more observations than those only available since 1991. All financial data are in thousands of real 2002–03 dollars per (current) pupil.

Unfortunately, we do not have access to statewide district-level student test score data throughout this period.[10] As an alternative quality measure, we use the continuation rate, defined as one minus the dropout rate, where the dropout rate is the number of dropouts identified at the school level by grade in each year, divided by the enrollment in grades 7–12 for the district.[11] Dropout data are available from 1991 through 1999. We transform the continuation rate to be the number of continuing students per 1000, using the Common Core of Data for the total number of students enrolled in public school and enrollment by grade.[12] Merging districts went from 991 students per 1000 continuing in 1991 to 989 in 1999, while nonmerging districts went from 987 to 989.

Table 1 presents summary statistics for our key variables in 1991 and 2001, by whether or not the district experienced a merger during this period. There are no significant differences between the boundary areas that do and do not experience mergers in either 1991, indicating no immediately obvious selection into consolidation, or in 2001.

4. Results

We present results for the baseline specification (equation [1]), which identifies the average treatment effect of consolidation on an area (regardless of whether the consolidation was covered by state financial incentives or was preceded by WGS), the comparison between consolidation and WGS (equation [2]), and the specification allowing eligibility for incentives and time since merging to affect outcomes (equation [3]). Results for school inputs and outcomes (the pupil-teacher ratio, mean school enrollment in the district, the dropout rate, and a cost-adjusted dropout rate) are in table 2, revenues in table 3, and expenditures in table 4. In all specifications we control for district and year fixed effects and district-specific time trends.

4.1 School Inputs and Student Outcomes Results

Improvements in quality are often cited as the main reason to consolidate. We consider two input measures commonly thought to be correlated

Table 1
Descriptive Statistics

Descriptive Statistics[1]				
	1991		2001	
Merger between 1991 and 2001	Merger	None	Merger	None
	(1)	(2)	(3)	(4)
Locally generated revenue PP	2.803	2.544	3.897	3.864
	(0.495)	(0.551)	(0.736)	(0.903)
Local property tax revenue PP	2.289	2.050	3.041	2.897
	(0.441)	(0.475)	(0.605)	(0.778)
State revenue PP	2.336	2.333	3.760	3.950
	(0.262)	(0.280)	(0.423)	(0.651)
Federal revenue PP	0.215	0.213	0.340	0.377
	(0.050)	(0.083)	(0.121)	(0.268)
Total current exp PP	4.672	4.496	6.885	6.889
	(0.387)	(0.407)	(0.584)	(0.716)
Current exp PP: instructional	2.938	2.905	4.213	4.194
	(0.303)	(0.270)	(0.381)	(0.466)
Current exp PP: non-instructional	1.734	1.591	2.672	2.695
	(0.205)	(0.219)	(0.326)	(0.396)
Current exp PP: physical plant maintenance	0.462	0.420	0.597	0.572
	(0.096)	(0.077)	(0.107)	(0.118)
Current exp PP: transportation	0.220	0.213	0.316	0.299
	(0.051)	(0.064)	(0.082)	(0.110)
Current exp PP: services	1.144	1.041	2.044	2.046
	(0.146)	(0.154)	(0.287)	(0.342)
Current exp PP: administration	0.407	0.360	0.597	0.657
	(0.089)	(0.094)	(0.105)	(0.160)
Surplus PP	0.683	0.594	1.112	1.302
	(0.310)	(0.352)	(0.477)	(0.904)
Pupil:teacher ratio	13.706	13.865	12.104	12.283
	(2.297)	(2.354)	(1.361)	(1.759)
Mean school enrollment	199.609	243.765	222.926	243.915
	(53.484)	(90.354)	(66.846)	(102.685)
Non-dropout rate (grades 7–12, per 1000)[2]	9.368	13.389	11.333	11.274
	(8.692)	(11.199)	(9.914)	(12.217)
Non-dropout rate/instructional exp PP[2]	3.214	4.662	3.031	3.031
	(2.945)	(3.944)	(2.653)	(3.152)
N	49	269	49	268

Sources: F-33 School District Financial Data Files, Common Core of Data, administrative merger data.

Note: PP = per pupil; exp = expenditure. [1]All fiscal variables are in thousands of real 2002 – 03 dollars per pupil. [2]Dropout rates for 1999 (most recent in compatible series) are used instead of for 2001.

with quality, the pupil-teacher ratio and mean school size, and one student outcome variable, the continuation rate for grade seven through twelve (the inverse of the dropout rate), at the district level for the relevant time period. In table 2, we consider the effects of consolidation on these variables, as well as on a cost-adjusted continuation rate. The coefficient on this variable in the baseline specification (column 10) should be interpreted as meaning that once two distinct districts have merged into one, 1.99 fewer students progress through high school (or 1.99 more students drop out of high school) per 1000 students, for each $1000 of current spending per pupil, where 1.99 is statistically indistinguishable from zero. The average effects of consolidation on all measures reported in table 2 are statistically insignificant, and the differential effects based on timing and eligibility of the consolidation are insignificant as well (columns 3, 6, 9, and 12).

What about effects of consolidation as compared to WGS? Columns 2, 5, 8, and 11 show that the point estimates for the effect of consolidation when controlling for WGS are roughly equal to the effect of consolidation without controlling for WGS plus the independent effect of WGS, confirming the intuition that our baseline specification is generally comparing consolidation to WGS rather than to a situation without any instructional consolidation. It is interesting to note, however, that the independent effects of WGS and at times consolidation when controlling for WGS approach statistical significance. For class and school size, the effects are in the expected direction: consolidation and/or WGS make both classrooms and schools have more students. The negative effect on the continuation rate (corresponding to a positive effect on high school dropout) is significant for WGS at the 10 percent level. This goes against the motivation that consolidation should improve quality, but is consistent with the motivation behind recent policy movement toward eliminating very large high schools. The negative effect on the cost-adjusted continuation rate, significant for both consolidation and WGS variables at the 10 percent level, is counter to the motivation that consolidation should improve efficiency.

It should be noted that we choose these measures based on data availability. As noted earlier, high school dropout is one of few education quality indicators on which smaller districts tend to outperform larger districts. It is possible that the types of quality indicators on which small districts systematically perform worse than larger districts would tell a different story. Perhaps, for example, more subjects are offered in high schools following consolidations. We cannot answer this question because of data

Table 2
Effects of Consolidation on School Inputs and Outcomes

	Dependent Variable[1]											
	Pupil:Teacher Ratio			Mean School Enrollment			Continuation Rate[2]			Continuation Rate/ Current Exp PP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Consolidated	0.712	1.169	0.374	6.593	12.780	−11.232	1.292	−0.963	1.073	−1.992	−18.405	−3.431
	(0.866)	(0.995)	(0.654)	(9.255)	(12.924)	(10.127)	(1.674)	(2.054)	(4.882)	(5.462)	(9.719)	(10.911)
Whole-grade sharing		0.452			6.126			−2.352			−17.118	
		(0.238)			(6.201)			(1.235)			(9.197)	
Merged 1-5 years ago			−0.066			1.689			1.786			0.312
			(0.339)			(5.997)			(3.387)			(4.422)
Merged between 1991 and 1993			1.487			26.316			4.485			9.012
			(2.578)			(28.329)			(6.251)			(14.967)
Merged within 5 years *merged 91-93			−0.464			2.114			−5.664			−4.903
			(0.776)			(11.537)			(4.217)			(6.451)
District fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
Year fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
District-specific time trends	yes		yes	yes		yes	yes		yes	yes		yes
Observations	4,438	4,438	4,438	4,759	4,759	4,759	2,768	2,768	2,768	2,768	2,768	2,768
R-squared	0.61	0.61	0.62	0.95	0.95	0.95	0.56	0.56	0.56	0.85	0.85	0.85

Sources: F-33 School District Financial Data Files for 1989–2001, administrative merger data.

Note: [1]All fiscal variables are in thousands of real 2002–03 dollars per pupil. [2]Continuation rate is estimated as 1000 minus the number of dropouts in grades 7–12 per 1000 students currently enrolled in grades 7–12. Robust standard errors in parentheses. *significant at 5%; ** significant at 1%.

constraints.[13] It is also possible that a longer time horizon would reveal greater impact.

4.2 Revenue Results

Table 3 presents the results of estimating the regressions specified in equations 1, 2, and 3 for several categories of revenue that we might expect to vary with consolidation. On average, column 1 shows that consolidation is associated with a reduction in local revenue (again, net of revenue received from other school systems) of about $257 per pupil, meaning that the consolidated district collects $257 less per pupil than the sum of the revenues collected by the two separate districts before they merged. Column 2 shows that the negative effect of consolidation on local revenue in column 1 really reflects a positive effect of WGS on local revenue, with consolidation having no significant independent effect. Column 3 shows that the total local revenue results in column 1 are determined solely by districts merging in the 1991–1993 eligibility window. Interestingly, this reduction appears to be permanent in the sense that the effect does not vanish after the five-year period of state subsidies. One interpretation of this result is that voters approved the mergers because of the state incentives but were less supportive of the larger district ex-post.

In terms of the composition of these changes in revenue, about two-thirds of this average local revenue response, or $177 per pupil, is from reductions in property tax revenue (columns 4 and 5). The reduction in property tax revenue is mechanical for districts merging in the period covered by state incentives, and column 6 suggests, albeit imprecisely, that nearly all of this reduction is explained by consolidations taking place within the state eligibility window of 1991 to 1993.

Reductions in local revenue collected in the first five years after an incentive-eligible merger do not mechanically reduce the amount of revenue available for local districts to spend because the state makes up the difference. State revenue to the district shows a significant impact on average, with districts receiving about $242 in additional state funds per pupil via this incentive, on a base of about $2,350 in state revenue per pupil in 1991. State revenue increases for districts eligible for incentives because it is calculated using additional weights for students formerly in whole-grade sharing arrangements only for consolidating districts; it also compensates for the lower foundation property tax rate. This larger transfer for consolidation, relative to WGS, is reflected in column 8. The greatest magnitude effect, as well as the only statistically significant one, is for districts

Table 3
Effects of Consolidation on Revenue Sources

	Total Local Revenue PP[2]			Local Property Tax Revenue PP			Total State Revenue PP			Total Federal Revenue PP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Consolidated	−0.257**	0.013	−0.003	−0.177**	−0.002	0.006	0.242**	0.526**	−0.079	−0.003	0.017	−0.025
	(0.069)	(0.101)	(0.126)	(0.054)	(0.087)	(0.094)	(0.048)	(0.073)	(0.076)	(0.008)	(0.012)	(0.019)
Whole-grade sharing		0.262**			0.170**			0.276**			0.019*	
		(0.072)			(0.064)			(0.049)			(0.008)	
Merged 1–5 years ago			−0.013			−0.013			0.115			0.024
			(0.096)			(0.066)			(0.066)			(0.015)
Merged between 1991 and 1993			−0.414**			−0.175			0.045			−0.010
			(0.157)			(0.109)			(0.118)			(0.027)
Merged within 5 years *merged 91–93			−0.033			−0.102			0.199*			−0.006
			(0.119)			(0.084)			(0.090)			(0.018)
District fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
Year fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
District-specific time trends	yes		yes	yes		yes	yes		yes	yes		yes
Observations	5,082	5,082	5,082	5,079	5,079	5,079	5,078	5,078	5,078	4,757	4,757	4,757
R-squared	0.89	0.89	0.89	0.91	0.91	0.91	0.96	0.96	0.96	0.63	0.63	0.63

Sources: F-33 School District Financial Data Files for 1986–2001, administrative merger data.

Note: PP = per pupil; exp = expenditure. [1]All fiscal variables are in thousands of real 2002–03 dollars per pupil. [2]Total local revenue excludes revenue from other school systems. Robust standard errors in parentheses. *significant at 5%; **significant at 1%.

merging while eligible and in the last five years (column 9). In terms of federal revenue, we find small increases associated with whole-grade sharing.

4.3 Expenditure Results

Table 4 examines the effect of WGS and consolidation on school district expenditures. In our regressions including only consolidation indicators, which implicitly compare consolidation to WGS, we find no significant expenditure responses. When separately controlling for WGS activity, however, we find several statistically significant results. For example, we find that WGS, relative to two separate districts, leads to an increase in total current expenditures of $213 per pupil (column 2). These effects are concentrated in non-instructional spending, such as plant, transportation, and services (columns 8, 11, 14, and 17). We find similar increases in each of these non-instructional spending components for consolidation although the corresponding effect on total expenditures is statistically insignificant. These increases in expenditures are smaller than the increases in revenues, however, and an increased surplus thus results (column 23). In terms of the timing of consolidation, we do not find any statistically significant differences between the different types (column 3).

4.4 Reconciling the Revenue and Expenditure Findings

Integration, regardless of whether it occurs via consolidation or via WGS, leads to increases in spending. Under WGS, this increase is financed by increases in both state and local aid. Under consolidation, by contrast, this increase is financed exclusively by increased state aid. These changes in expenditure, however, are smaller than the changes in revenues, and thus the increased state aid contributes to an increased surplus. These findings suggest that, in the absence of any associated quality improvements, integration, whether in the form of WGS or administrative consolidation, may lead to increased costs, at least in the short run. This could be due, for example, to increased transportation costs. This in turn suggests that the significant state incentives may be counterproductive in the sense that they potentially encouraged inefficient integration. There are two important caveats to this conclusion. First, we have measured short-run effects, and the long-run effects of integration are unclear and cannot be addressed here given that this activity occurred in the last twenty years. Second, our quality measures are not ideal and it could be that increased spending is

Table 4
Effects of Consolidation on Expenditures

	Dependent Variable[1]											
	Total Current Exp PP			Current Instructional Exp PP			Current Non-Instructional Exp PP			Current Exp PP: Physical Plant Maintenance		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Consolidated	−0.048	0.170	−0.142	−0.030	0.025	−0.014	−0.018	0.146**	−0.128*	0.015	0.064**	−0.025
	(0.063)	(0.093)	(0.134)	(0.044)	(0.066)	(0.091)	(0.032)	(0.045)	(0.064)	(0.011)	(0.018)	(0.023)
Whole-grade sharing		0.213**			0.053			0.160**			0.051**	
		(0.068)			(0.051)			(0.031)			(0.016)	
Merged 1–5 years ago			0.141			0.055			0.086*			0.020
			(0.072)			(0.045)			(0.040)			(0.012)
Merged between 1991			−0.240			−0.148			−0.093			0.045
and 1993			(0.178)			(0.129)			(0.086)			(0.036)
Merged within 5 years			0.044			−0.016			0.060			−0.015
* merged 91–93			(0.097)			(0.059)			(0.057)			(0.018)
District fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
Year fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
District-specific time trends	yes		yes	yes		yes	yes		yes	yes		yes
Observations	5,079	5,079	5,079	5,079	5,079	5,079	5,079	5,079	5,079	3,487	3,487	3,487
R-squared	0.95	0.95	0.95	0.93	0.93	0.93	0.92	0.92	0.92	0.78	0.78	0.78

(continued)

Table 4
(continued)

	Dependent Variable[1]											
	Current Exp PP: Transportation			Current Exp PP: Services			Current Exp PP: Administration			Surplus PP		
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Consolidated	−0.008	0.029*	0.020	0.009	0.128*	−0.008	0.004	0.002	0.006	0.031	0.382**	0.036
	(0.009)	(0.014)	(0.015)	(0.026)	(0.055)	(0.046)	(0.015)	(0.026)	(0.025)	(0.063)	(0.116)	(0.130)
Whole-grade sharing		0.039**			0.124*			(0.002)			0.341**	
		(0.011)			(0.052)			(0.022)			(0.103)	
Merged 1–5 years ago			−0.017			0.003			0.002			−0.015
			(0.012)			(0.030)			(0.019)			(0.073)
Merged between 1991			−0.049*			−0.061			−0.029			−0.138
and 1993			(0.022)			(0.063)			(0.031)			(0.154)
Merged within 5 years			0.023			0.060			0.014			0.114
*merged 91–93			(0.014)			(0.037)			(0.021)			(0.089)
District fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
Year fixed effects	yes		yes	yes		yes	yes		yes	yes		yes
District-specific	yes		yes	yes		yes	yes		yes	yes		yes
time trends												
Observations	4,445	4,445	4,445	3,487	3,487	3,487	3,487	3,487	3,487	5,082	5,082	5,082
R-squared	0.97	0.97	0.97	0.93	0.94	0.93	0.89	0.89	0.89	0.82	0.82	0.82

Sources: F-33 School District Financial Data Files for 1986–2001, administrative merger data.

[1]All fiscal variables are in thousands of real 2002–03 dollars per pupil.

Note: PP = per pupil; exp = expenditure. Robust standard errors in parentheses. *significant at 5%; **significant at 1%.

associated with increases in quality; this would be consistent with consolidation advocates' arguments, which generally center on quality rather than costs.

4.5 Other Effects of Consolidation: Areas for Future Research

We attempted to estimate the role of consolidation on several outcomes not presented in this article: enrollment, private schooling, residential property values, and high school curricular offerings. We found no effects of consolidation on total enrollment in the public schools. Instead of using private school enrollment, which requires estimating which districts are sending students to which private schools (the majority of school districts do not contain a private school within their boundaries), we examined changes in public school enrollment within cohorts from year to year (due to changes in private school enrollment or inter-district residential mobility), and found no significant effects. Regarding property values, we concluded that the declining agricultural economy was too great an influence, and the agricultural composition of each school district too poorly measured, to consider property values as an outcome. Finally, we obtained administrative data on the share of high school students enrolled in given curricular subjects in secondary school, but there missing data was a sufficiently large problem to prohibit further analysis.

5. Conclusions

The consolidations in Iowa in the 1990s—prompted at considerable state expense—did not change pupil-teacher ratios, enrollments, or dropout rates. If these outcomes were not realized in Iowa, where the consolidating districts were quite small, it is unlikely that encouraging consolidation in states with larger districts would do any better. While promoting consolidation without cost would appear harmless, these consolidations were quite costly to state taxpayers. It is important to note, however, that we lack the most appropriate outcome variables in this study—student test scores—so while we observe no quality changes in the provision of education given the available data, it is possible that other measures of quality were improving, reflecting improved efficiency from consolidation.

In terms of fiscal responses, we find that both WGS and administrative consolidation lead to higher state aid and that this effect is not offset by reductions in local revenue; in fact, we find that WGS leads to increases in

local revenue. We find corresponding increases in local spending under both WGS and consolidation; these increases in spending, however, are smaller than increases in revenue, resulting in an increased surplus.

In addition to these effects, the state subsidies redistribute income from all state taxpayers to those in more rural and agriculture-dependent consolidating districts (during the eligibility window). Just under 5 percent of Iowa's population in 1989 lived in a district that would consolidate while eligible for the incentives, between 1991 and 1993. Mean household income for those districts was $29,191 (in 1989 dollars), compared with $31,765 in districts that never received the fiscal incentives; that difference is not statistically significant.

Given the lack of observable efficiency gains at hand, however, we can interpret the value of state fiscal incentives to a given district as an upper bound of the value initial districts placed on their independence. While these districts did not choose to merge before the incentives were offered, the magnitude of the state incentives did prompt them to consolidate, presumably compensating them for reductions in autonomy through the reduction in the foundation property tax rate and the increase in state formula aid for the first five years following consolidation.

Much of the debate surrounding school district consolidation relies on the assumption that changing the district boundaries will result in reductions in school size. When previous whole-grade sharing arrangements had achieved similar effects without consolidating districts, the impact of consolidation on district outcomes is necessarily focused on district-level administrative functions rather than school-level inputs. More broadly, discussions of optimal jurisdictional scale rely on those jurisdictions being the unique providers of public goods to their residents. In that sense, our case is limited in its ability to inform us further with regard to the topical policy question of optimal *school* scale. These issues should be considered in determining the optimal jurisdictional level for the provision of other local public goods with multiple "plant" locations within a jurisdiction, such as fire or police stations.

Notes

1. The question of optimal jurisdictional size is far from restricted to school districts. The tradeoffs between economies of scale in local publicly provided goods production versus control over local decisions have been investigated with regard to nations (Alesina and Spolaore 1997), cities and suburban annexation (Austin 1999), and school districts and municipalities (Brasington 1999; Alesina, Baqir, and Hoxby 2004). This literature has focused more on

explaining district formation and consolidation (as we do in Gordon and Knight [2006]), rather than on estimating gains from scale. The literature on education cost functions addresses the issue of scale more directly; see Andrews, Duncombe, and Yinger (2002) for an overview.

2. Many states enacted legislation mandating or providing strong financial incentives for districts to consolidate over the course of the twentieth century; see Hooker and Mueller (1970) for an overview of such legislation. In a national study, Kenny and Schmidt (1994) show that these state incentives were key factors in determining local school district organization.

3. Elected school boards decide when to impose enrichment levies, and do not require direct voter approval to do so. They hold for five years, and can be renewed by the board for a second five-year term, after which voter approval is required. If voters disagree with the taxing decisions of the school board, they can petition to require a popular election in the school district to approve the new rate. In nearly all cases, however, districts continue using the instructional support levy for a second five-year term.

4. Specifically, by consolidating, districts with enrollments of fewer than 600 students before consolidating could lower their foundation tax rate from the mandated 5.40 mills to 4.40 mills in the first year post-consolidation, increasing by 0.20 mills per year until reaching 5.40 again in the sixth year after consolidation, where it would remain.

5. If we view the decision to consolidate as a choice between WGS and consolidation, districts may have chosen not to consolidate in earlier years to retain their supplemental weights. This reason not to consolidate is not valid for mergers effective after 1993 (although they would still receive greater benefits from merging between 1991 and 1993), so may help to explain why more districts than average consolidated even after the greatest financial incentives were no longer applicable. Increased consolidation activity post-1993 could also be because of districts still responding to the farm crisis and enrollment decline.

6. For districts that did not participate in any consolidations from 1986-2001, the initial and final boundaries are identical and no aggregation is required.

7. As discussed in the section on the specification of our analyses, all such regressions use year-level fixed effects to capture time trends.

8. There are two cases of multiple mergers over the time period. In each of the cases, two small districts merged early in the period, and then later merged with a larger district. We code these mergers as taking place during the year of the later merger, but show robustness checks to changing this definition.

9. There are two unusual types of reorganizations worth noting. First, there were two cases of a single school district being dissolved and its students distributed across multiple districts. In the first case, it is clear that although the district (Hedrick) technically was dissolved into multiple districts, nearly all of its (small) enrollment was evident in an increase in one of those districts (Pekin) the next year. Because this dissolution was involuntary, we choose not to treat it as a merger between the two districts and instead drop the Hedrick school district from the data. In another case, Grand Valley, the district had only forty-four students enrolled the year before being dissolved. Five districts were designated as recipients of their students, but because the enrollment of the original district was so low, it is not possible for us to attribute change in the receiving districts' enrollments to the dissolution of Grand Valley. Again, we drop the dissolved district from our sample in all post-dissolution years. There were also two cases in which a school district, as it existed in 1989, went through two consolidations from 1989 to 2001. In all other cases, there was only one consolidation over the period.

10. The University of Iowa, which maintains district-level scores on the Iowa Test of Basic Skills, will not release them without the permission of each individual district.

11. The dropout data may be accessed at: http://www.state.ia.us/educate/fis/pre/eddata/hist/index.html. These data differ from the diploma recipiency rate that one can calculate with the diploma recipient count variable reported in the Common Core of Data.

12. We attempted to perform some analyses using enrollment by race and ethnicity, the number of free-lunch eligible students, special education students, and limited English proficiency students, all available through the Common Core, but found consolidating districts were generally similar in their composition, consistent with the homogeneity of Iowa as a state.

13. While data on the number of students enrolled in particular subjects by district are available for one year preceding the consolidation movement and for one year after the bulk of activity, these data are insufficient as many small districts offer subjects such as chemistry and physics every other year, so it is not possible to tell a true zero from an "off year" zero.

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